GOVERNMENT DISTORTION IN INDEPENDENTLY OWNED MEDIA: EVIDENCE FROM U.S. NEWS COVERAGE OF HUMAN RIGHTS

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Abstract
This study provides evidence of government distortion of news coverage among independently owned media outlets in a democratic regime. It uses data from 1946 to 2010 and documents that U.S. news coverage of human rights abuses committed by foreign governments was associated with membership on the United Nations Security Council and the degree of political alliance with the United States. For countries that were not allied with the United States, coverage increased with membership; for countries that are strongly allied with the United States, coverage decreased with membership. There is an analogous effect on reports of human rights abuses by the U.S. State Department, but no such effect on human rights practices according to other measures. The results are driven by the Reagan and Bush Sr. Administrations, 1981–1992, a period during which the government was known to have actively influenced the press. (JEL: P16, N4)

1. Introduction

Mass media is believed to play a powerful role in democracies. It is often referred to as *the fourth estate*. It reaches an immense audience, and its content can affect a wide

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range of outcomes, including political behavior such as voting.[1] However, the ability of the media to perform its prescribed role as the "watchdog" of democracy has come under question as observers point to an increasing number of instances when media content is distorted by the government.[2] Bennett et al. (2008, p. 8) summarizes the motivation behind these concerns: "The democratic role of the press is defined...by those moments when government deception or incompetence compels journalists to find and bring credible challenges to public attention and hold rulers accountable. This accountability function of the U.S. press has been weakened in the contemporary era, and its standing is sorely in need of greater examination".[3]

This paper attempts to make progress on this important question by assessing the extent of government distortion of the news in the United States. Our study aims to determine whether the anecdotal and case evidence on government manipulation reflects isolated incidents or systematic distortion that could be a symptom of deeper and more fundamental concerns. In other words, we ask whether a democratic government can systematically distort news coverage from independently owned outlets. This question has received very little attention in the literature thus far.

Our study proceeds in three steps. First, to motivate our investigation, Section 2 documents that the U.S. government often attempted to influence news coverage of human rights practices of their political allies. We rely on qualitative evidence from political scientists as well as internal government memos that explicitly state government objectives and tactics for a large number of cases. The bulk of the evidence come from memos that were declassified as part of the Iran-Contra investigation and mostly pertain to the last decade or so of the Cold War.

Second, Section 3 provides a conceptual framework for the empirical analysis. We combine the recent theories of endogenous news coverage developed by Prat and Stromberg (2005, 2011) and Stromberg (1999, 2004a,b) with a theory of media manipulation by Besley and Prat (2006) to provide one plausible explanation of why or how the U.S. government manipulates news and to provide empirically testable implications of whether there is any government distortion in our context. We also discuss alternative explanations to our results after we present them in Section 7.

1. Recent studies have shown that media can affect voting behavior (e.g., Prat and Stromberg 2005; Gentzkow 2006; DellaVigna and Kaplan 2007; Chiang and Knight 2011; Enikolopov, Petrova, and Zhuravskaya 2011; Adena et al. 2013), other political behavior (Gerber, Karlan, and Bergan 2009; Paluck 2009; Olken 2009), and social outcomes such as literacy (Gentzkow and Shapiro 2008) female empowerment (Jensen and Oster 2009) and fertility (La Ferrara, Chong, and Duryea 2008).

2. Numerous books written by political scientists and former journalists voice this concern. Prominent examples include Bennett, Lawrence, and Livingston (2008), Cook (1998), and Thomas (2006). Also see the works referenced in Bennett et al. (2008) and Cook (1998) for the large body of work about the media and the U.S. government from media and political science scholars.

3. The quote from Pulitzer was originally printed in the *North American Review* (1904). More controversial works documenting the influence of the U.S. government on the media include the well-known work of Chomsky (2011), where he compares the compliance of the American media with U.S. government's directives to that of the *Pravda* and the Russian government.

Finally, the principal contribution of our study is to estimate the causal effect of political alliance with the United States on news coverage of human rights abuses in five large U.S. newspapers (*New York Times*, *Washington Post*, *Chicago Tribune, Los Angeles Times*, and *Wall Street Journal).* The two main difficulties are the measurement of news coverage and alliance, and causal identification. To measure news coverage, we conduct text analysis to construct the number of news articles of human rights abuses for each country in each year. As a proxy for alliance with the United States, we use the fraction of votes that a country makes in agreement with the United States in United Nations General Assembly resolutions over which the United States and the Soviet Union (Russia, post 1991) disagree. Our data include all of the years for which we were able to conduct text analysis, 1946–2010. Our use of voting in the UN General Assembly as a proxy of U.S. alliance follows Alesina and Dollar (2000) and Qian and Yanagizawa-Drott (2009).[4]

The difficulty in causal identification arises from the fact that alliance and news coverage of human rights abuses can be jointly determined by a third omitted variable. For example, Civil War may both reduce the value of a country as an ally to the United States and worsen its human rights abuses, which would result in more news coverage. Alternatively, there may be reverse causality. For example, more news coverage about human rights abuses may reduce the willingness of the U.S. government to ally itself with a foreign nation. To address these two problems, we use a *difference-in-differences* strategy and estimate the interaction effect of our proxy for alliance and rotating membership in the United Nations Security Council. The logic of the strategy is that allies are strategically valuable to the U.S. government, and this value increases when they are on the Council because they are able to vote on important United Nations (UN) resolutions.

Our empirical strategy builds on insights from two earlier works. Kuziemko and Werker (2006) find that Council membership can significantly increase a foreign country's strategic value to the U.S. government. Qian and Yanagizawa-Drott (2009) finds that the U.S. State Department reports allies as having better human rights records than nongovernmental organizations such as Amnesty International report them to have. To address the possibility that voting patterns in the General Assembly may be endogenous, we use a two year lagged measure. The baseline specification includes country fixed effects, which control for all time-invariant characteristics across countries such as cultural affinity with the United States that can affect the degree of alliance, Council membership, and/or news coverage; and year fixed effects, which control for any changes over time that influence all countries equally. In addition, it controls for the interactions of alliance and the full vector of year fixed effects and the interactions of Council membership with the full vector of year fixed effects. These additional controls allow the importance of alliance and Council membership to vary fully flexibly over time. The main hypothesis is that if the U.S. government reduced

4. Alesina and Dollar (2000) finds that voting with the United States in the General Assembly is positively correlated with U.S. foreign aid receipts. We discuss Qian and Yanagizawa-Drott (2009) later in the Introduction.

news coverage of the human rights abuses of countries that were strategically important, then the interaction effect of Council membership and alliance will be negative.

We find that for the full sample (1946–2010), the interaction effect of Council membership and alliance is negative and statistically significant. The result is robust to several alternative measures of alliance, including lagged military alliance, whether the legal origin of a country was European but not socialist or communist, and lagged military aid from the United States. As a placebo exercise, we show that controlling for the interaction of Council membership and U.S. military aid, the interaction of Council membership and U.S. economic aid has no effect.

Next, we investigate whether the effects are more prominent during the Reagan and Bush Sr. administrations, which is the focus of the anecdotal evidence of government manipulation. We find that this is indeed the case. There is no significant effect before, from the Truman to Carter administrations, or afterward, during the Clinton and Bush Jr. administrations.

Thus, our results are consistent with the presence of government distortions during 1981–1992. This interpretation relies on the assumption that there were no other forces that were simultaneously correlated with news coverage, Council membership, and political alliance with the United States. We do not take this as given and provide a large body of evidence in support of our interpretation. First, we show that the timing of our reduced form effects corresponds with entry onto and exit from the Council. Second, we show that the response of annual U.S. State Department reports of human rights abuses of foreign countries to the interaction of alliance with the U.S. government and UN Security Council (UNSC) membership is very similar to that of news coverage, which is consistent with our interpretation that the interaction effect captures the influence of U.S. government manipulation. Third, we show that Council membership does not have similar effects on reports by Amnesty International or several proxies of human rights behavior and institutional quality. These results go against the alternative explanation that our results reflect changes in actual human rights practice. Finally, we show a significant and negative effect on the number of articles about human rights abuses as a fraction of total articles about a given country. This goes against the alternative that our results are driven by changes in overall news coverage. See Section 6 for a detailed discussion.

For policymakers, our findings send a mixed message. On the one hand, they confirm the case evidence from the 1980s that systematic government-driven distortions existed for independently owned and highly competitive media outlets in a democratic regime. On the other hand, the fact that we only observe distortions during two presidential administrations may be viewed optimistically as indicating that such distortions cannot exist indefinitely in the U.S. context. For example, Gentzkow et al. (2015) find that the government in power has little influence over news composition in the historical U.S. context.

In our focus on the government's influence of media coverage, our study is most closely related to Besley and Prat (2006). It also builds directly on the pioneering work of Besley and Prat (2006), Durante and Knight (2012), Prat and Stromberg (2005, 2011), and Stromberg (1999, 2004a,b) by adapting the frameworks developed in these papers and applying them to a novel empirical context. To the best of our knowledge, our paper is the first to provide rigorous evidence that government distortion has systematically existed in the United States. In doing so, we add to the important empirical literature on the determinants of news coverage. Our study differs from previous studies by focusing on government-driven distortions in a democratic regime. In this sense, we are related to recent studies that find evidence of government influence of the media in Italy under Silvio Berlusconi (Durante and Knight 2012) and in Argentina through government advertising DiTella and Franceschelli (2011).

Second, we add to the small but growing number of political economy studies that explore the causes and consequences of U.S. government foreign policy. In our focus on the Cold War era, our study is most closely related to Dube, Kaplan, and Naidu's (2011) study of U.S. covert actions on U.S. firm stock prices, and Berger, Easterly, and Satyanath's (2009) and Berger et al.'s (2012) studies of U.S. Cold War policies on trade. It broadens the scope of this literature by examining the effect of U.S. foreign policy on the American public. In using the degree to which a country votes with the United States in the UN General Assembly on issues for which the United States votes in opposition to the Soviet Union to proxy for alliance, we build on our earlier paper, Qian and Yanagizawa-Drott (2009). The current paper differs from Qian and Yanagizawa-Drott (2009) in studying the additional advantage of Council membership and examining news coverage as an outcome.

Finally, our examination of United Nations Security Council members builds on earlier works by Kuziemko and Werker (2006) and Alesina and Dollar (2000) that were discussed earlier. In using Council membership as a proxy for a country's strategic value to the United States, we build on Kuziemko and Werker (2006), which finds that Council membership in years that are strategically important for the U.S. government results in higher U.S. aid. They proxy for the importance of the year with the number of *New York Times* articles about the Security Council and then estimate the interaction effect of this measure and Council membership on foreign aid from the United States. Our study complements theirs in examining news coverage of human rights abuse as the outcome.

This paper is organized as follows. Section 2 discusses the background. Section 3 summarizes the conceptual framework. Section 4 presents the empirical strategy. Section 5 describes the data. Section 6 presents the main empirical results. Section 7 discusses alternative interpretations. Section 8 offers concluding remarks.

2. Background

2.1. "White Propaganda" During the Reagan and Bush Sr. Administrations

The main period of our study, 1981–1992, was characterized by a commitment to fight communism on the part of the American government, which climaxed during the Reagan administration (1981–1988) and continued with the Bush Sr. administration (1989–1992). As with all of the Cold War, rivalry between the two superpowers was expressed through military coalitions, propaganda, and proxy wars (e.g., the Soviet war in Afghanistan 1979–1989). The Cold War ended in 1991 when the U.S.S.R. dissolved.

An important feature of the Cold War in the United States was the focus on the superior morality of the West. The U.S. government and news media often described its allies as “good” and the Eastern Bloc and its allies as “evil”. Recently declassified files stored in the U.S. National Security Archives document both the method and the motives for the U.S. government to influence the press coverage of the human rights practices of its political allies.

The government needed public support for its political actions, which included public approval of its political allies. Given the focus on morality, it followed that U.S. allies should have better human rights practices than Eastern Bloc allies.[5] For the most part, U.S. government support for its Cold War allies with poor human rights abuses ended with the Cold War. Internal memos show that the executive branch believed that one of the ways to shape public opinion against opponents was to exaggerate human rights abuses in those countries and emphasize that among other things, they were “evil”, practiced “forced conscription” or engaged in the “persecution of the church”. Conversely, the government attempted to increase support for political allies by calling them “freedom fighters”, “religious”, or simply “good” (Jacobwitz 1985b).

During the Reagan administration, the task of influencing press coverage was officially delegated to the *Office of Public Diplomacy* (OPD). The OPD was part of the State Department and worked closely with the National Security Council (NSC). Its explicit purpose was to influence public and congressional opinion to garner support for the President’s strong anticommunist agenda in a “public action” program (Parry and Kornblub 1988). The memo specifies that audiences for the information campaign include the U.S. media (Jacobwitz 1985b).

> “...we can and must go over the heads of our Marxist opponents directly to the American people. Our targets would be within the United States... the general public [and] media.”—Kate Semerad, an external relationship official at the Agency for International Development (AID) in 1983.

Government methods for influencing the media can be broadly categorized into two groups. First, the government can attempt to directly manipulate news reports by exerting pressure on editorial boards or incentivizing journalists. The OPD monitored news reports by the American media and would directly confront journalists and editors in order to convince them to change the reports (Schultz 1984). Upon the appearance of news reports that did not conform to the wishes of the OPD, officials could press the

5. For studies on U.S. government favoritism of human rights reports of its Cold War allies, see studies such as Carleton and Stohl (1985), Mitchell and McCormick (1988), Poe, Carey, and Vazquez (2001), and Qian and Yanagizawa-Drott (2009).
In the case of the *The New York Times,* which published an international version under the title of *The International Herald Tribune*, manipulation could also affect the opinion of foreign readers. Also, influencing the press could also affect congressional opinion, whose favor was often necessary for legislative purposes (Blanton and Blanton 2007).

owners and editorial boards to change their journalists in the field. The OPD also dealt directly with journalists using a carrot-and-stick strategy. For example, uncooperative journalists became the targets of character assassination meant to induce skepticism about the information they reported and were sometimes even forcibly removed from foreign countries from which they were reporting.[6] In contrast, journalists seen as cooperative to the administration's agenda were rewarded with increased access to government information. For example, an OPD memo stated that certain favorable correspondents had "open invitations for personal briefings" (Cohen 2001).[7]

Second, the government can manipulate the supply of information and provide disinformation. Information can be disseminated through the numerous government affiliated publicity events and publications. One such publication is the *Country Reports on Human Rights Practices*, which we will discuss later in the paper. In a letter to House Speaker Patrick Buchanan, the Deputy Director for *Public Diplomacy for Latin American and the Caribbean* (SLDP), Jonathan Miller, described how the OPD was carrying out "white propaganda" operations. This included writing opinion articles under false names and placing them in leading newspapers such as the *Wall Street Journal* (Hamilton and Inouye 1987; Miller 2001). Similar opinion editorials were planted in the *New York Times* and the *Washington Post* (Brooks 1987). The OPD paid extra attention to prominent journalists. In general, the OPD flooded the media, academic institutions and other interested groups with information. For example, in 1982, the OPD booked more than 1,500 speaking engagements with editorial boards, radio, and television interviewers, distributed materials to 1,600 college libraries, 520 political science faculties, 122 editorial writers, and 107 religious groups (Parry and Kornblub 1988).

2.2. The United Nations

The UN, a source of much of the diplomatic influence and the principal outlet for the foreign relations initiatives of nonsuperpower countries, was especially important during the Cold War.[8] Two of the five principal organs of the UN are the General Assembly and the Security Council. During the period of our study, there were approximately 150 member states, of which more than two-thirds were developing countries. The General Assembly votes on many resolutions brought forth by sponsoring states. Most resolutions, while symbolic of the sense of the international

6. One famous case was the removal of *New York Times* reporter Raymond Bonner from El Salvador after his unfavorable reporting of the massacre by the Salvadoran government. The U.S government pressured the NYT to recall Bonner (Parry and Kornblub 1988). Other outlets such as the *Wall Street Journal* subsequently published articles criticizing the NYT for publishing Bonner's reports.

7. Blanton and Blanton (2007) provides an overview of all the actions taken by the OPD during the Reagan Administration. For detailed accounts of when the media allows the government to distort reports, see Bennet, Lawrence, and Livingston (2007) and Thomas (2006). Also, see Latham (2012) for a study of how career concerns affect intelligence reports in the CIA.

8. For a detailed discussion of the history and institutions of the United Nations, see Malone (2004).

community, are not enforceable as a legal or practical matter. The General Assembly does, however, have authority to make final decisions in areas such as the UN budget, and in case of a split vote in the Council when no veto is exercised, the issue goes for a vote in the General Assembly.

The Security Council comprises 15 member states. Council members have more power than General Assembly members because the Council can make decisions that are binding for all UN member states, including economic sanctions and the use of armed force (Chapter Seven of the UN Charter). There are ten temporary seats that are held for two-year terms, each one beginning on January 1st. Five are replaced each year. The members are elected by regional groups and confirmed by the UN General Assembly. New members are typically announced the year before the term begins.[9] There are five permanent members (P5): China, France, Russia, the United Kingdom, and the United States. These members hold veto power for blocking adoption of a resolution. Experts vary in their assessment of the power of rotating members over important issues during our period of study. On the one hand, rotating members cannot overturn vetoes and some political scientists argue that they have limited real power (e.g., O'Neill 1996). On the other hand, studies such as Voeten (2001) argue that P5 countries prefer multilateral agreements, which, in turn, gives much power to rotating members. For example, deadlocks on the Council can only occur if there is no veto and nine of the ten deadlocks that have ever occurred in the history of the UN occurred during the Cold War.[10]

Rotating membership was standardized to be two years for all members. Prior to that, membership was typically two years (a small minority, ten members, had one year terms). Members are elected one year prior to entry (Malone 2000, p. 5).

3. Conceptual Framework

In the Online Appendix, we develop a framework of how alliance with the U.S. government and membership on the United Nations Security Council can interact to affect U.S. news coverage of human rights abuses of foreign countries. The goals of our model are to provide one plausible explanation for how the government influences human rights news coverage of foreign countries and to derive testable implications to guide the empirical investigation of whether there is government distortion in the

9. Africa elects three members; blocs such as Latin America and the Caribbean, Asia, and Western Europe choose two members each; and the Eastern European bloc chooses one member. Also, one of these members is an Arab country, alternately from the Asian or African bloc. Members cannot serve consecutive terms, but are not limited in the number of terms they can serve in total. There is often intense competition for these seats (Malone 2000).

10. 1956 Suez Crisis; 1956 Soviet Invasion of Hungary (Hungarian Revolution); 1958 Lebanon Crisis; 1960 Congo Crisis; 1967 Six Days War; 1980 Soviet invasion of Afghanistan; 1980 Israeli-Palestinian Conflict; 1981 South African occupation of Namibia (South West Africa); 1982 Israeli Occupation of the Golan Heights (Golan Heights Law); 1997 Israeli-Palestinian conflict (East Jerusalem and Israeli-occupied territories).

context that we study. In particular, our model aims to provide an internally consistent explanation for why Council membership could decrease the amount of news coverage for strongly allied countries, while increasing the amount of coverage for countries that are not allied. After we present the empirical results, we discuss alternative explanations in Section 7.

Specifically, our framework studies the incentives of the government to distort media coverage of state repression in foreign countries. In our model, domestic voters care in part about the foreign policy that the current U.S. administration pursues, but voters cannot directly and fully evaluate the foreign policy (preferences) of the incumbent. Voters partly base their inferences on the behavior of allied foreign countries that vote with the United States in the UN. News reports about human rights violations of the allies serve as indicators of U.S. foreign policy, which affects the probability that the incumbent U.S. administration will be voted out of the office.

Here, we sketch the basic intuition behind the model, which is formally presented in the Appendix. In our model, "worse" countries are more likely to commit human rights violations and are more likely to vote with the United States if the current administration's foreign policy is bad (from the perspective of U.S. voters). U.S. voters observe voting behavior and read about human rights violations to make their inferences about the current administration's type. There are two groups of voters. The first group is interested in and reads the news about all foreign countries. As a result, voters in the first group make inferences based on the behavior of all countries. The second group is interested only in the countries that are currently on the UNSC. We do not formally model the reason for this. This assumption is motivated by the fact that the Council discusses more important issues and/or has more power over these issues. Alternatively, it could simply be because being on the Security Council acts as a focal point for readers with limited interest in foreign policy. The second group solely bases its inferences on the news coverage of Council members.

In our model, obtaining a seat on the Security Council generates two effects on news coverage. The first is a *demand effect*. As a country becomes a member of the Council, more people are interested in reading about it. In the absence of government interference, newspapers would then increase their coverage of human rights abuses in these countries. The second effect is a *distortion effect* that comes from the incentives of the government to manipulate the media. We show that if the number of countries not on the Council is much larger than the number of countries on the Council (e.g., the General Assembly), it is much cheaper for the U.S. government to manipulate public opinion by suppressing news about Security Council countries than non-Security Council countries. Because voters in the first group based their inferences on voting and human right violations of all countries, distorting news coverage about one of them has little effect on the posterior beliefs of this group when the total number of countries is large. In contrast, the voters in the second group base their inferences on the voting behavior of a relatively small number of countries on the Council, and the distortions in the coverage of one country has a large effect on the voters' posterior beliefs about the current administration's type. As a result, when the country enters the Council, it is optimal for the U.S. government to significantly intensify its distortion of news coverage. Moreover, this effect is monotone. The closer the foreign country is aligned with the United States, the more severe the distortion will be.

Methodologically, our approach combines recent theories of endogenous news coverage developed by Prat and Stromberg (2005, 2011) and Stromberg (1999, 2004a,b) with a theory of media manipulation by Besley and Prat (2006). That voters in our model try to infer the quality of the government policies from news reports and that newspaper coverage affects the posterior beliefs of the voters about the quality of those policies is very similar to Durante and Knight (2012).[11]

4. Empirical Strategy

The relationship between news coverage, U.S. alliance and Council membership can be characterized as the following log-linear relationship:

$$Y_{it} = \alpha + \beta(A_{it} \times C_{it}) + \delta X_{it} + \gamma_i + \theta_t + \varepsilon_{it}, \tag{1}$$

where the outcome variable, news coverage of human rights abuses, in country i in year t, Y_{it}, is a function of: the interaction of alliance to the United States, A_{it}, and membership on the Security Council, C_{it}; a vector of country-year specific controls, X_{it}; year fixed effects, δ_t; and country fixed effects, γ_i. Since the number of news articles is a count variable and there are many observations with the value of zero, we estimate this model using a poisson regression.[12] The standard errors are clustered at the country level to adjust for serially correlated shocks within countries. The country fixed effects control for all time-invariant differences across countries. Year fixed effects control for changes over time that affect all countries similarly. $\boldsymbol{X_{it}}$ includes a vector of country-year controls, such as alliance interacted with year fixed effects and Council membership interacted with year fixed effects. These controls allow the importance of alliance and Council membership to change over calendar years. Note that since countries enter the Council on different calendar years, these controls are not collinear to the main interaction term.

The identification strategy is conceptually similar to a *differences-in-differences* (DD) strategy. We compare outcomes for countries when they are on the Council to when they are not, between countries that are strongly allied to the United States to those that are less allied. β is the differential association of Council membership and news coverage between countries that are not allied at all, $A_{it} = 0$, and countries that are "perfectly" allied, $A_{it} = 1$. The goal of our empirical exercise is to test whether $\hat{\beta} < 0$.

11. Durante and Knight (2012) study the optimal choice of news outlet based on their ideological leaning. We abstract from the differences in ideology and focus on the incentives of the government to manipulate news coverage.

12. Online Appendix Table A.7 also presents the results from when we use OLS to estimate a log-linear specification.

Interpreting the association between the interaction effect and news coverage as causal requires the assumption that Council membership does not differentially affect allies in some way that will influence news coverage through channels other than U.S. government distortion or reader demand. Specifically, for the interaction term to overstate the true degree of government distortion, the omitted factor needs to reduce the increase in news coverage when a country enters the Security Council and the reduction needs to be increasing with the level of political alliance with the United States. For example, if improvement in human rights practices when entering the Council is positively correlated with alliance, then the interpretation of our estimates will be confounded. We will carefully consider this and other robustness concerns after we present the main results.

5. Data

This paper uses data that are constructed from numerous publicly available sources. For brevity, we only describe the data for the main analysis in this section, which covers the period 1946–2010. Other data will be discussed as they become relevant.

News coverage of human rights violations is measured as the number of newspaper articles about human rights abuse in a given country. Following the definitions used by *Freedom House* and the *Political Terror Scale* project, we define human rights as physical violence committed by the state onto civilians. We calculate the number of articles based on a search of the text of articles in the *ProQuest Historical and National Newspapers* database. Our measure of human rights coverage is the total number of articles that results from the search per country per year. The newspapers we examine are *The New York Times* (NYT, available 1946–2010), *The Washington Post* (WP, available 1946–1997), *The Wall Street Journal* (WSJ, available 1946–1996), *The Chicago Tribune* (available 1946–1990), and *The Los Angeles Times* (L.A. Times, available 1946–1990). These are the only newspapers within the ten highest circulation papers for which we could conduct a full text search for the main period of our study.

Our measure includes both articles written by journalists employed by newspapers and stories picked up from newswires and other sources, although the newspapers in our sample, and in particular the NYT and *Washington Post*, were known for original international news reporting.[13] This does not affect the interpretation of the results, but for completeness, we also examine the impact on articles from newswires after we present the main results.

The most simple search algorithm would count the number of articles with a country's name and the phrases "human rights" or "human rights abuse". The main difficulty with this procedure is that it may capture articles that do not criticize the country's human rights abuses. For example, it may be an article commending the

13. It is not possible to use automated text analysis to accurately and systematically distinguish between articles written by different sources.

improvements of human rights (i.e., the lessening of abuses) in a given country. This could induce measurement error in our dependent variable. As long as the error structure is classical, it should not bias our results. Nevertheless, to minimize such measurement error, we impose additional constraints by searching for articles containing the country's name, the phrase "human rights" and require at least one of the words or phrases that fall under the *UN Declaration for Human Rights* (and that are therefore also commonly used in news articles on human rights abuse). These include "torture", "violations", "abuse", "extrajudicial", "execution", "arbitrary arrests", "imprisonment", and "disappearances". The logic is that journalists who wish to write about human rights abuse will very likely research the official definition of human rights violations and then either consciously or unintentionally use similar language as the official document.[14]

Our main proxy of alliance with the United States is the fraction of votes that a country votes in agreement with the United States on issues for which the United States votes in opposition to the Soviet Union (Russia, post 1991) in the UN General Assembly, A_{it}, where $A_i \in [0, 1]$.[15] This is constructed from resolution level data. We first identify the resolutions over which the United States and the Soviet Union (Russia, post 1991) voted in opposition. Then, we calculate the fraction of such resolutions that a given country voted in agreement with the United States. If a country abstains, then we code the vote as a missing value. Thus, our main measure of alliance excludes abstentions.[16] To avoid potential endogeneity in contemporaneous voting patterns, we use a two-year lag since Council membership typically lasts two years. We will show that our main result is qualitatively robust to several alternative measures of alliance.

14. This constrained search could still include measurement error. For example, if an article names several countries in a region, but only criticizes the human rights abuses of one of these countries (by including one of the search phrases we use), then our search algorithm will result in one article about human rights abuse for each of the countries named in the article. We know of no automatized way to completely avoid such measurement error when conducting a large scale automatic search and explored whether this is likely to be a major problem in two ways. First, we examined the country names and years of the 200 observations with the most coverage (this is approximately 10% of the 1,937 observations that have any news coverage in our sample). All of these are countries and years with known human rights abuses according to Amnesty International. Online Appendix Table A.6 lists the top 100 observations during the Reagan and Bush administrations for brevity. Second, we read a random sample of articles. Given logistical constraints and the purpose of making sure that our results are not driven by measurement error, we randomly selected 200 articles from the Reagan and Bush administrations (1981–1992). This is slightly less than 5% of the total number articles. We did not find any evidence that our measure overcounts the number of articles about human rights abuses. Note that, as before, if this measurement error of the dependent variable is classical, then it will not bias our estimates.

15. We do not examine voting patterns in the Council because most issues are discussed prior to being put onto the agenda. Therefore, the samples of issues voted on are not representative of the actual issues being deliberated by Council members.

16. In Online Appendix Table A.5, we show that our results are qualitatively robust to an alternative measure where abstentions are coded as voting against the United States. The table also shows that our results are qualitatively robust to using a dummy variable for alliance, where a country is allied if our main measure of alliance is above the sample median.

Data on Council membership are collected from *The United Nations Security Council Membership Roster.*[17] This is a time-varying dummy variable for whether a country is a rotating member of the UN Security Council, C_{it}. The final sample of countries excludes former Soviet Republics that did not have membership in the United Nations before 1991 and South Africa, which was excluded from UN activities due to the UN's opposition to apartheid and therefore did not vote on resolutions in the UN General Assembly for the majority of the time period we study. The five permanent members of the Council are also excluded since they do not experience any variation in Council membership.

The main data for news coverage and Council membership includes the maximum number of years for which we could obtain news data, 1946–2010. The number of countries in the panel grows from 36 in 1946 to 113 in 2010.[18]

The data show that a country received an average of almost two articles about its human rights abuses each year, with the average coverage being much higher during the Reagan and Bush administrations at 3.5 articles. There is significant variation in coverage across observations ranging from no coverage to a maximum of 103 articles in a single year (El Salvador in 1982). For any time period, 6–7% of the samples are UNSC members.[19] The data also show that approximately 32% of observations have at least one news article published on human rights in a U.S. newspaper. This peaks during the Reagan and Bush administrations at 46%.

We will also use data on human rights abuses as reported by the U.S. government. We describe this data more in detail in Section 6.6.1.[20]

6. Results

6.1. Main Results

In this section, we present the estimates of equation (1). We begin by using the full sample, which covers 1946–2010. Table 1 presents our main results on news coverage.

17. See http://www.un.org/sc/list_eng5.asp for a list of all countries that were ever members and the years of their memberships. 85 out of the 115 countries in the sample were on the Council as a rotating member at least once during this time.

18. Online Appendix Table A.1 lists the Council members for the period of 1981–1992, the years of membership, the level of alliance, and the number of news articles on human rights. Online Appendix Table A.6 lists the 100 observations with the most human rights news coverage during 1981–1992. We limit the lists to this time period for brevity, since we will later show that the news distortions occur for the Reagan and Bush administrations.

19. See Online Appendix Table A.2 for descriptive statistics by time period.

20. Online Appendix Figure A.1 illustrates news coverage of human rights abuses over time. It shows that the average number of news articles across countries and the total number of news articles for all countries both begin to increase in 1972 and stay high until the early 1990s. The variation across countries increases during the same period. The two vertical lines indicate the beginning and end of the Reagan and Bush administrations (1981 and 1992) that coincide with the Cold War, which we will later show to be the period driving our main results.

TABLE 1. The effect of UNSC membership and alliance on news coverage.

	Dependent variable: no. of news articles about HR abuse						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample restriction	Full sample	Full sample	Baseline, full sample	Excluding: strongest/weakest allies, 10th percentile	Excluding: strongest/weakest allies + Never on UNSC	Excluding: strongest/weakest on UNSC + Top 5% HR articles	Full sample
Dependent variable mean	**2.10**	**2.10**	**2.10**	**2.29**	**2.42**	**1.77**	**2.10**
UNSC × Alliance	−1.32	−2.94	−3.14	−4.45	−4.16	−4.14	
	(0.708)	(1.044)	(1.521)	(2.107)	(1.723)	(1.597)	
Alliance	−0.12	−0.02					
	(0.421)	(0.416)					
UNSC	0.13						0.18
	(0.164)						(0.240)
UNSC × Medium Ally							−0.14
							(0.301)
UNSC × Strong Ally							−0.50
							(0.285)
Country and Year FE	Y	Y	Y	Y	Y	Y	Y
UNSC × Year FE	N	Y	Y	Y	Y	Y	N
U.S. Alliance × Year FE	N	N	Y	Y	Y	Y	N
Observations	5,767	5,767	5,767	5,191	3,913	3,774	5,767
Clusters	116	116	116	116	86	86	116
Alliance SD	0.215	0.215	0.215	0.124	0.126	0.126	–
Effect for One SD Alliance	−24.7%	−46.8%	−49.1%	−42.5%	−40.8%	−40.6%	–
Mean Alliance for Weak Allies	–	–	–	–	–	–	0.0009
Mean Alliance for Medium Allies	–	–	–	–	–	–	0.0746
Mean Alliance for Strong Allies	–	–	–	–	–	–	0.449

Notes: All columns are estimated using poisson regressions. The standard errors are clustered at the country level, with the number of clusters reported in each column. The percentage effect at the bottom of the table is calculated as exp(Beta×Alliance SD)−1, where Beta refers to the UNSC × Alliance Coefficient. In column (7), the omitted category is the interaction of UNSC × Weak Ally.

The sample means of the dependent variables are presented at the top of the table. Our main measure of news coverage is the number of news articles about country i's human rights abuses during year t across all newspapers. In column (1), we present the estimates for the uninteracted U.S. alliance and Council membership variables. The former is negative, whereas the latter is positive. However, they are both statistically insignificant. The interaction of Council membership and alliance is negative and statistically significant at the 10% level. In column (2), we interact Council membership with the full vector of year fixed effects to address the possibility that the relationship between the U.S. government and its allies and the importance of Council membership changed over time. The interaction effect becomes −2.94 and statistically significant at the 1% level. Column (3) additionally interacts alliance with the vector of year fixed effects. This is our baseline specification. The interaction coefficient becomes −3.14 and is statistically significant at the 5% level.[21] Taken literally, the coefficient implies that Council membership will reduce the number of reports by 3.14 log points more for countries that vote with the United States all of the time relative to countries that never vote with the United States.

Since no country in the sample actually votes with the United States all of the time, a more meaningful comparison is for countries with alliance measures that are one standard deviation (SD) apart. At the bottom of the table, we show that one standard deviation of the alliance measure in the sample used for the regression is 0.215. Thus, relative to a country that votes with the United States on 21.5 percentage points fewer issues, Council membership reduces the number of articles on human rights abuses by 49% ($\exp^{-3.14\times 0.215} - 1 = 0.491$).

In column (4), we check that our estimates are not driven by outliers by omitting observations that are in the top or bottom tenth percentile of the sample distribution of alliance.[22] The interaction coefficient remains negative, is slightly larger in magnitude than the full sample estimate in column (3), and is statistically significant at the 5% level. In column (5), we additionally exclude countries that were never on the Council to check that our estimates are not driven by spurious correlations with these countries. The estimate is similar to before. In column (6), we also exclude observations in the top five percentile of the distribution of articles about human rights for each country. This checks that our estimates are not driven by extreme values in the data. Again, our estimate is similar to the full sample baseline in column (3). We conclude that our

21. One may be concerned that there is very little variation in the baseline specification after controlling for the large number of fixed effects (and interactions with fixed effects) in the baseline specification. We examine this by using OLS to estimate the baseline specification with all of the variables other than the interaction of Council membership and alliance. The R-square is 0.63 (the adjusted R-square is 0.59). This means that 37% (or 41%) of the variation in news coverage is still unexplained. Moreover, it is only a slight increase from the less restrictive specification in column (1), where we do not control for the interactions of Council membership and alliance with year fixed effects. In that case, the OLS estimates produce a R-square of 0.62 (adjusted R-square of 0.58).

22. Since approximately 25% of the sample do not vote with the United States on divided votes, this restriction effectively only drops the top 10% allies.

results are not sensitive to outliers in alliance or news coverage, and are indeed driven by the countries that have been on the Council at least once.

In column (7), we estimate an alternative specification to examine the relationship between alliance and UNSC membership across alliance levels. For this exercise, we divide countries into three equally sized groups according to how closely they are allied to the United States. We then interact the medium allied and most strongly allied dummy variables with UNSC membership. The reference group is the uninteracted UNSC membership effect, which captures the effect of Council membership for the least allied group of countries (i.e., the average country in terms of alliance). To allow for enough variation in the estimate, we do not interact alliance or Council membership with the year fixed effects.[23] The uninteracted Council member effect is 0.18 and statistically insignificant, which means that Council membership on average has little effect for news coverage for the least allied group. The interaction with the medium allied group is −0.14. The sign change relative to the least allied group is consistent with Council membership reducing news coverage for stronger allies. However, the lack of statistical significance means that the effect is indistinguishable from zero. The interaction coefficient for the strongest allied group is −0.5 and significant at the 10% level. The three interaction coefficients suggest that the effect of Council membership on news coverage is roughly monotonic across alliance levels.

Finally, in Online Appendix Table A.7, we show that the results of Table 1 are robust to using OLS to estimate a log-linear specification.

6.2. *Alternative Measures of Alliance*

In principle, there are many possible proxies for strategic and political alliance with the United States. We choose to use voting patterns in the General Assembly as our main proxy because the estimates are relatively easy to interpret. However, our estimates are also robust to several other measures. Table 2 presents the estimates with three alternative measures.[24] At the bottom of the table, we present the mean and standard deviation of each alliance measure. These show that there is substantial variation in all measures.

Column (1) restates our main baseline results. In column (2), we use time varying measures of military alliance based on the Correlates of War military alliance measure. “The Correlates of War Formal Alliance data set seeks to identify each formal alliance between at least two states that fall into the classes of defense pact, neutrality, or nonaggression treaty, or entente agreement. A defense pact (Type I) is the highest level of military commitment, requiring alliance members to come to each other’ s aid

23. In practice, when we interact Council membership and alliance with year fixed effects as in the baseline specification, the coefficients are nearly identical. The results are available upon request.

24. Online Appendix Table A.3 presents the correlations across the various measures, and Online Appendix Table A.4 lists the countries and their average level of alliance with the United States for each measure during 1981–1992, the period that we will later show to drive our main results.

TABLE 2. The effect of UNSC membership and alliance on news coverage—robustness to alternative measures of alliance.

	Dependent variable: no. of news articles on human rights abuse			
	(1)	(2)	(3)	(4)
Dependent variable mean	**2.097**	**2.097**	**2.946**	**2.098**
UNSC ×				
UNGA Vote$_{t-2}$ (Main Measure)	−3.138			
	(1.521)			
Military Alliance$_{t-2}$		−0.119		
		(0.0674)		
Legal Origin			−0.856	
			(0.347)	
Log U.S. Military Aid$_{t-2}$				−0.0656
				(0.0355)
Log U.S. Economic Aid$_{t-2}$				0.0207
				(0.0400)
Observations	5767	5767	3920	5,763
Clusters	116	116	116	116
Alliance Mean	0.159	−0.003	0.894	0.354
Alliance SD	0.215	1.780	0.307	1.754
Effect for One SD Alliance	−49.1%	−19.1%	−23.1%	−10.9%
Years in Sample	1946–2010	1946–2010	1946–2010	1948–2010

Notes: All columns are estimated using poisson regressions and include baseline controls—UNSC Membership × Year FE, Alliance × Year FE, Year FE, and Country FE. The standard errors are clustered at the country level, with the number of clusters reported in each column. The mean and standard deviation of each alliance measure are reported at the bottom of the table. In column (4), the alliance mean and standard deviation are reported for U.S. Military Aid. The percentage effect at the bottom of the table is calculated as exp(Beta×Alliance SD)−1, where Beta refers to the UNSC × Alliance Coefficient. The sample years are stated at the bottom of the table. They and the number of observations vary according to the availability of the alliance measures.

militarily if attacked by a third party. As the labels imply, neutrality and nonaggression pacts (Type II) pledge signatories to either remain neutral in case of conflict or to not use or otherwise support the use of force against the other alliance members. Finally, ententes (Type III) provide for the least commitment and obligate members to consult in times of crisis or armed attack. Each alliance classifies the highest level of military support that an alliance member pledges to another alliance member".[25] For each year, the database reports the type of alliance between each country and the United States, as well as the Soviet Union (Russia). For example, 26.7% of the sample is engaged in Type I, 27.2% of the sample is engaged in Type I or II, and 28.4% of the sample is engaged in any military alliance with the United States. For brevity, we incorporate all of the information from these variables by taking the first principal component of the three U.S. alliance and three Soviet alliance variables and using it as another proxy for alliance. To avoid endogeneity, we use a two-year lagged measure of this variable.

25. See http://www.correlatesofwar.org/COW2%20Data/Alliances/alliance.htm.

Column (2) shows that the interaction of this proxy is also negative and statistically significant at the 10% level.

Another proxy is based on the legal origin variable created by La Porta et al. (1999). This variable identifies whether the Company Law or Commercial Code has its origins from English common law, French commercial code, socialist/communist laws, German commercial code, or Scandinavian code. Our proxy for alliance equals one if a country has its legal origins in Europe, but not from socialist/communist laws (i.e., the origins are from one of the four other categories). This variable is time-invariant. Column (3) shows that the interaction of this time-invariant proxy is negative and statistically significant at the 5% level.

In column (4), we use U.S. military aid, measured in log 2012 constant USD, to proxy for alliance. As with our other time-varying proxies, we use a two year lagged measure to avoid endogeneity. This measure is motivated by the belief that the United States gives more military aid to its strategic allies. In the same equation, we control for the two year lag of U.S. economic aid, also measured in log 2012 USD. The interaction of military aid and UNSC is negative, large in magnitude, and statistically significant at the 10% level. This result is consistent with our hypothesis. In contrast, the interaction of U.S. economic aid and UNSC has no effect, which is interesting, since it suggests that nonmilitary U.S. aid is not being disproportionally sent to allied countries. In this sense, economic aid is a placebo alliance measure. The results support our interpretation.

The results in columns (1)–(4) show that our main finding is qualitatively robust to several proxies of alliance, which supports our interpretation that our main measure, lagged General Assembly voting patterns, captures the effect of alliance. Note that as before, we present the implied effect of a one standard deviation difference in alliance at the bottom of the table. Interestingly, the magnitude of the relative reduction in news coverage due to Council membership is broadly similar across the different measures of alliance, ranging from −11% to −23%.

Henceforth, we will focus on our main measure.

Note that the sample sizes vary across the estimations in Table 2 because of the different availability of the alliance proxies.

6.3. The Reagan and Bush Cold War Administrations

Given the large body of evidence of government manipulation of human rights coverage toward the end of the Cold War discussed in Section 2, one may naturally wonder whether the Reagan and Bush Sr. administrations drive the full sample results. In this section, we investigate whether the extent of government distortion varies over time.

We take an agnostic ex ante approach and begin by dividing the data into two crude periods: the Cold War and the post-Cold War eras. Table 3 shows the baseline specification estimates for different time periods. Panel A (column (1)) restricts the sample to the Cold War period. The interaction coefficient is similar in magnitude to the full sample period and statistically significant at the 10% level. Column (2) examines

TABLE 3. The effect of UNSC membership and alliance on news coverage for different time periods.

	Dependent variable: no. of news articles on human rights abuse									
	(1) CW 1946–1991	(2) Post-CW 1992–2010	(3) CW republican 1946–1991	(4) CW democrat 1946–1991	(5)	(6)	(7)	(8)	(9)	(10)
					A. Panel A					
Dependent variable mean	**2.31**	**1.75**	**2.74**	**1.72**						
UNSC × Alliance	−2.82	−1.29	−6.79	−0.52						
	(1.65)	(6.03)	(2.63)	(1.21)						
Observations	3,625	2,142	2,084	1,541						
Clusters	114	114	114	110						
Alliance SD	0.237	0.049	0.232	0.235						
Effect for One SD Alliance	−48.8%	−6.1%	−79.3%	−11.5%			–			
					B. Panel B					
	Truman and Eisenhower 1946–1960	JFK and LBJ 1961–1968	Nixon, Ford, and Carter 1969–1980	Carter and Reagan 1977–1988	Reagan 1981–1988	Reagan and Bush 1981–1992	Reagan and Bush CW 1981–1991	Clinton 1993–2000	George W. Bush 2001–2008	Omit Reagan and Bush 1946–1980, 1993–2010
Dependent variable mean	**0.30**	**0.18**	**2.15**	**4.60**	**4.50**	**4.27**	**4.42**	**2.11**	**1.39**	**3.354**
UNSC × Alliance	0.60	−1.76	0.93	−3.87	−7.92	−9.23	−8.77	2.97	2.52	0.21
	(1.20)	(2.65)	(1.24)	(2.17)	(2.35)	(2.53)	(2.60)	(8.63)	(5.40)	(1.38)
Observations	554	627	1,197	1,338	907	1,359	1,247	900	904	4,520
Clusters	55	91	110	114	114	114	114	116	116	116
Alliance SD	0.299	0.271	0.171	0.115	0.114	0.082	0.080	0.047	0.049	0.236
Effect for One SD Alliance	19.6%	−37.9%	17.3%	−36.0%	−59.4%	−53.1%	−50.6%	15.0%	13.2%	5.1%

Notes: All columns are estimated using poisson regressions and include the baseline controls: UNSC membership × Year FE, Alliance × Year FE, Year FE, and Country FE. The percentage effect at the bottom of the table is calculated as exp(Beta × Alliance SD)−1, where Beta refers to the UNSC × Alliance coefficient. The standard errors are clustered at the country level, with the number of clusters reported in each column.

the post-Cold War period. The interaction effect is small in magnitude and statistically insignificant. Thus, our findings are specific to the Cold War. This is consistent with anecdotal evidence. For example, the president of Zaire (renamed the Democratic Republic of Congo in 1997), Mobutu Sese Seko (in office 1965–1997) was a strong supporter of the United States during the Cold War and had been repeatedly criticized for human rights abuses. However, during a state visit to the United States in 1983, United States president Ronald Reagan responded to these criticism by stating publicly that Mobutu was a "voice of good sense and good will". Immediately after the Cold War ended, the State Department began to criticize Zaire's human rights violations. In 1993 Mobutu was denied a visa for visiting the United States and said "I am the latest victim of the Cold War, no longer needed by the United States. The lesson is that my support for American policy [now] counts for nothing" (Gbadolite 2001).

Next, we take another crude cut of the data and investigate whether the effects for the Cold War period varies between Republican and Democratic administrations. Column (3) shows that the interaction coefficient for Republican administrations is large in magnitude, negative, and statistically significant at the 1% level. Column (4) shows that there is no effect for Democratic administrations during the Cold War. Thus, our results are specific to Republican administrations during the Cold War.

From these results, it makes sense to investigate which of the Republican executive administrations during the Cold War matter the most for our results. To be comprehensive, Panel B presents results for all administrations. Note that in several cases, there is too little variation for us to estimate our baseline estimates for one administration alone. In such cases, we group the administration with either the preceding or subsequent one.

Columns (1)–(3) show that there is little effect from Truman to Carter. In Column (3), we group the Carter administration with the Nixon and Ford administrations and find that the interaction coefficient is positive, small in magnitude, and statistically insignificant. Column (4) shows that when we group the Carter administration with the Reagan administration, the interaction effect becomes negative and significant at the 10% level. When we restrict the sample in column (5) to only the Reagan administration, the interaction coefficient stays negative, increases in magnitude, and is significant at the 1% level. This, together with the estimates in columns (3) and (4) suggest that the interaction coefficient is not significantly negative during the Carter administration, and the effect begins to take place during the Reagan administration.

In column (6), we group the Reagan and Bush administrations. The interaction coefficient is negative, statistically significant at the 1% level, and statistically similar in magnitude as the Reagan administration. If anything, the coefficient increases in magnitude. Thus, the effects we find during the Reagan administration continues during the Bush administration. In column (7), we omit 1992, the last year of the Bush administration and one year after the official end of the Cold War. The estimate changes little. Thus, later, we will examine all years of the Reagan and Bush administrations and not distinguish between the Cold War and post-Cold War periods.

In columns (8) and (9), we examine the post-Cold War administrations of Clinton and Bush Jr. The interaction coefficient becomes positive, small in magnitude, and statistically insignificant. This implies that our earlier finding that there is no effect after the Cold War is true in both the democratic and republican post-Cold War administrations.

In column (10), we examine all years except the Reagan and Bush Sr. administrations (1981–1992). The estimate is nearly zero in magnitude and statistically insignificant. This is consistent with our other results that favoring allies on the Council with relatively fewer human rights news articles is a feature of the two Republican administrations at the end of the Cold War.[26]

To interpret the magnitude of the effect, we again present the SD of the alliance measure for each period at the bottom of each column in Table 3. The estimate and SD of alliance in Panel B (column (6)) imply that during the Reagan and Bush Sr. administrations, Council membership reduced news coverage of human rights abuses by 59% more for a country that voted with the United States by one standard deviation of the sample alliance.

In Table 4, we repeat the estimates in Tables 1 and 2 for the Reagan and Bush administrations and show that the results for this period are also robust to alternative sample restrictions and measures of aid. Interestingly, note that column (1) shows that the uninteracted Council effect is positive and significant during this period. This implies that during the period that drives our main results, news coverage increased with Council membership for unallied states, whereas the large negative interaction coefficient shows that it declined with Council membership for strongly allied states.[27] Interpreted with our model, the former is consistent with the presence of a demand effect, and the latter is consistent with the presence of government distortion.

6.4. Timing of the Effect

Interpreting the association between the interaction of UNSC membership and U.S. alliance and news coverage as a causal relationship assumes that conditional on the baseline controls, there are no other factors that are simultaneously correlated with UNSC membership and the degree of political alliance with the United States that can also affect news coverage. Specifically, for the interaction term to overstate the true degree of government distortion, the omitted factor needs to reduce the increase in news coverage from Council membership according to the level of political alliance

26. The statistically significant estimate for the Reagan–Bush period holds also if one corrects for multiple hypothesis testing across periods. In particular, only the Reagan–Bush estimate survives the Benjamin–Hochberg False Discovery Rate procedure at the 5% level.

27. We note that the interactions of Council membership with the medium alliance and strong alliance dummy variables in column (7) is statistically insignificant. This is likely due to the reduction in sample size. However, the signs and magnitudes of the coefficients are consistent with the full sample results in Table 1 that the effect of Council membership on news coverage is likely to be monotonically decreasing with alliance.

TABLE 4. The effect of UNSC × years since Council Membership on news coverage during the Reagan and Bush Sr. administrations—robustness to sample restrictions and alternative alliance measures.

	Dependent variable: no. of news articles about HR abuse									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample restriction	Full sample	Full sample	Baseline, full sample	Excluding: strongest/ weakest allies, 10th percentile	Excluding: strongest/ weakest allies + never on UNSC	Excluding: strongest/weakest allies + never on UNSC + top 5% HR articles	Full sample	Full sample	Full sample	Full sample
Dependent variable mean	**4.269**	**4.269**	**4.269**	**4.469**	**5.328**	**4.822**	**4.269**	**4.269**	**4.269**	**4.269**
UNSC × Alliance	−3.946	−8.048	−9.230	−10.90	−9.654	−11.58				
	(1.607)	(2.309)	(2.528)	(4.537)	(4.887)	(4.937)				
Alliance	1.944	2.410								
	(1.876)	(1.777)								
UNSC	0.531						0.44			
	(0.217)						(0.25)			
UNSC × Medium Ally							−0.24			
							(0.36)			
UNSC × Strong Ally							−0.36			
							(0.34)			
UNSC × Military Alliance$_{t-2}$								−0.138		
								(0.0815)		
UNSC × Legal Origin									−0.922	
									(0.415)	
UNSC × Log U.S. Military Aid$_{t-2}$										−0.0549
										(0.0312)
UNSC × Log U.S. Economic Aid$_{t-2}$										0.0106
										(0.0381)
Country and Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
UNSC × Year FE	N	Y	Y	Y	Y	Y	N	Y	Y	Y
U.S. Alliance × Year FE	N	N	Y	Y	Y	Y	N	Y	Y	Y
Observations	1359	1359	1359	983	363	286	1359	1359	1359	1359
Clusters	114	114	114	113	44	41	114	114	114	114
Alliance SD	0.0820	0.0820	0.0820	0.0535	0.0523	0.0527	–	0.532	0.233	1.511
Effect for One SD Alliance	−27.6%	−48.3%	−53.1%	−44.2%	−39.6%	−45.7%	—	−7.1%	−19.3%	−8.0%
Mean Alliance for Weak Allies	–	–	–	–	–	–	0.00576	–	–	–
Mean Alliance for Medium Allies	–	–	–	–	–	–	0.0583	–	–	–
Mean Alliance for Strong Allies	–	–	–	–	–	–	0.185	–	–	–

Notes: All columns are estimated using poisson regressions. The standard errors are clustered at the country level, with the number of clusters reported in each column. In columns (8)–(10), controlling for the interaction of Year FE × alliance refers to the alliance measure that is interacted with UNSC as stated in the table. The percentage effect at the bottom of the table is calculated as exp(Beta×Alliance SD)−1, where Beta refers to the UNSC × Alliance coefficient. In column (7), the omitted category is the interaction of UNSC × Weak Ally. The mean and standard deviation of each alliance measure are reported at the bottom of the table. In column (10), the alliance standard deviation is reported for U.S. Military Aid. The sample includes 1981–1992. Additional restrictions are stated in the column headings.

with the United States. For very strongly allied countries, this omitted factor needs to cause Council membership to result in less coverage. In the remaining empirical exercises, we provide evidence that our estimates are unlikely to be driven by such omitted variables.

First, we show that the main results are unlikely to be driven by spurious correlations. The most direct way to do so is to estimate the effect for each year prior to and after a country is on the UNSC, an equation similar to our baseline, except that instead of a dummy variable indicating Council membership, we create eight dummy variables for two years before Council membership is announced, one year before it is announced, the year it is announced, each of the two years of Council membership, one year afterward, two years afterward, and three years afterward. We control for the same set of baseline controls as in equation (1):

$$y_{ist} = \sum_{s=-3}^{4} \alpha_s C_{ist} A_{it} + \sum_{s=-3}^{4} \beta_s C_{ist} + \Gamma X_{ist} + \delta_t + \theta_i + \varepsilon_{ist}. \tag{2}$$

The number of articles for country i, s years since it has been on the Council, during calendar year t is a function of: dummy variables for the number of years since it has been on the Council, C_{ist}, interacted with alliance, A_{it}; C_{ist}; a vector of controls, X_{ist}, which includes the interaction of calendar year dummies with alliance and with Council membership; year fixed effects, δ_t; and country fixed effects, θ_i. As with the baseline estimate, we estimate this log-linear relationship with a poisson regression and cluster the standard errors at the country level.

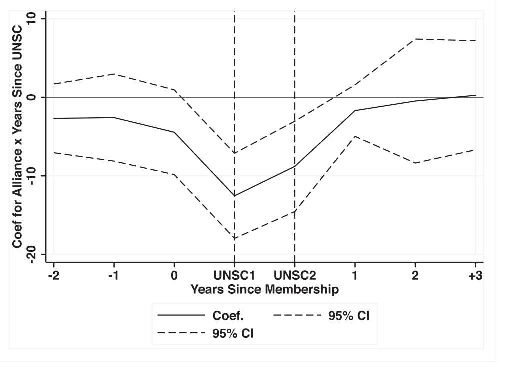
Since Council memberships are announced one year in advance and the announcement often results in news coverage of the country that includes discussion about its human rights practices, the effects could, in principle, begin the year before Council membership.[28] To reduce the noise in these estimates, we use the restricted sample that omits the strongest and weakest allies as in Table 1 column (4).

We present these results for several time periods, as motivated by the results from Table 3. First, we examine the main period driving the results. Figure 1(a) plots the interaction coefficients, $\hat{\alpha_s}$, and their 95% confidence intervals.[29] This figure shows that there is little differential effect prior to Council membership or afterward, but allies experience a relative reduction in coverage during Council membership. We observe no evidence of pre-trends.

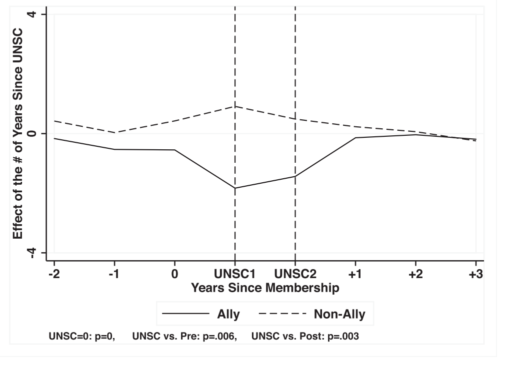
To observe the effect of membership on the level of coverage for allies and nonallies separately, Figure 1(b) plots the predicted effects for countries that are strongly allied

28. Note that in the main analysis, the Council membership dummy variable takes a value of zero for the year of the election. Thus, if the announcement results in news coverage of human rights abuses, our main results will be attenuated.
We currently exclude the countries with one-year terms in the pre-1965 period. The results are nearly identical with their inclusion. These are available upon request.

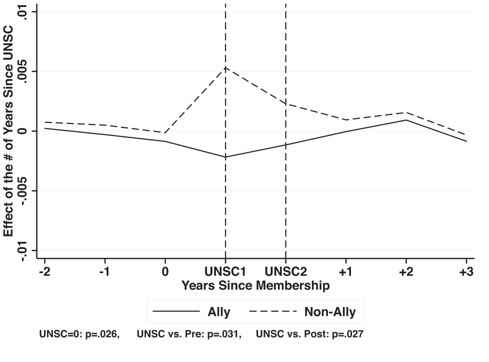
29. The interaction coefficients and their standard errors are presented in Online Appendix Table A.8.



(a) The Coefficients for UNSC × Years Since Council Membership



(b) The Predicted Effects of Council Membership for 90th and 10th percentile Allies on # of Articles about Human Rights Abuses



(c) The Predicted Effects of Council Membership for 90th and 10th percentile Allies on the Fraction of All Articles about Human Rights Abuses

FIGURE 1. The effect of UNSC × years since Council membership on news coverage during the Reagan and Bush Sr. administrations.

(votes with the United States are equal to the 90th percentile of the sample distribution) and countries that are weakly allied (votes with the United States are equal to the 10th percentile of the sample distribution) for the Reagan and Bush Sr. administrations.[30] The figure shows that news coverage for strongly and weakly allied countries is similar before and after Council membership. However, coverage diverges when these countries are on the Council. Coverage increases for weakly allied countries, whereas it declines for strongly allied countries.

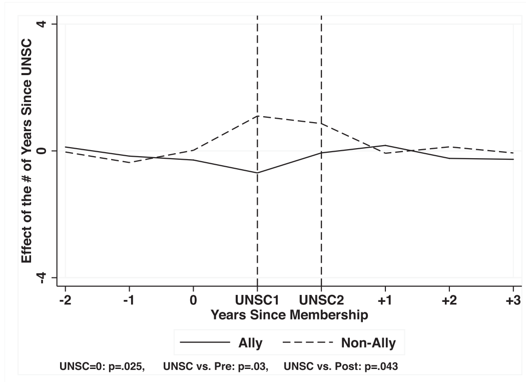
To formally examine whether the pre-Council and post-Council years statistically differ from the Council years, we test to see whether the interaction coefficients for each of those periods differ from the interaction coefficients of the Council years (see Appendix Table A.8 for the coefficients). The p-values for the two joint tests are reported at the bottom of Figure 1(b). They show that we can reject the null that the pre- and post periods are similar to the Council years. Note that we also present the p-value for the test of whether the interaction coefficients are statistically different from zero for the Council years. This is conceptually identical to the main exercise that examines the interaction of alliance and a Council membership dummy variable. We will return to discuss Figure 1(c) later in the paper.

We can also examine the timing of the effect in other periods. For brevity, we focus on the predicted effects of Council membership for the 10th and 90th% allies.[31] In Figure 2, we examine all years, the Truman to Carter administrations (1946–1980), the Clinton administration (1993–2000), and the Bush Jr. administration (2001–2008). The results for the full sample are unsurprisingly similar to the Reagan and Bush Sr. administrations. We find little difference between allies before, during or after Council membership for these periods. Note that for the Truman through Carter period, the predicted effects seem to diverge for the year prior to membership. However, the p-values at the bottom of the figure show that the interaction coefficients for the pre-council period are statistically indistinguishable from the Council period.

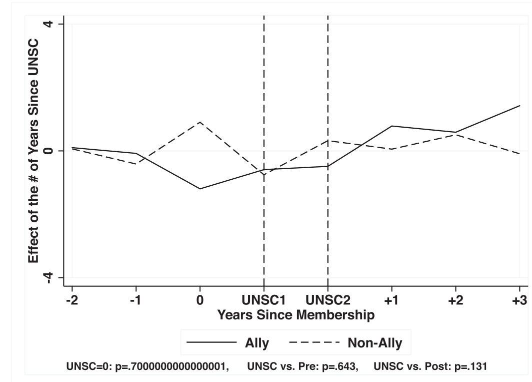
The figures in this section support the results in Table 3 that the distortion we detect is driven by the Reagan and Bush Sr. administrations. Henceforth, we focus on this period for brevity. More importantly, the timing of the effect supports our identification strategy and mitigates concerns that our results are driven by spurious correlations.

30. These are calculated using the interaction coefficients. For allies, we plot $\bar{v}\hat{\alpha}_s + \hat{\beta}_s$, where $\bar{v}$ is the 90th percentile of the distribution in our sample of how often a country votes with the United States on divided votes in the Council. For nonallies, we plot $\underline{v}\hat{\alpha}_s + \hat{\beta}_s$, where $\underline{v}$ is the 10th percentile of the distribution in our sample of how often a country votes with the United States on divided votes in the Council.

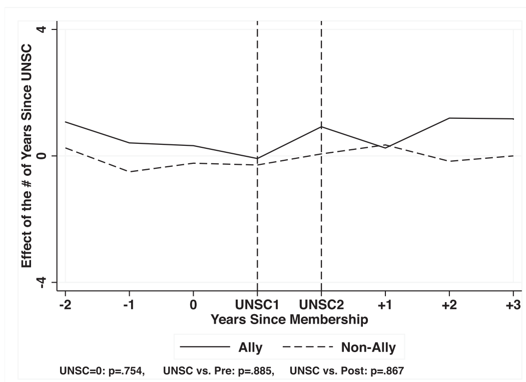
31. The interaction coefficients used to calculate the predicted effects are shown in Online Appendix Table A.8; and like in the baseline estimates in Table 1, they use the sample of allies within the 10th and 90th percentiles. They and their 95% confidence intervals are plotted in Online Appendix Figure A.5(a)–(d).



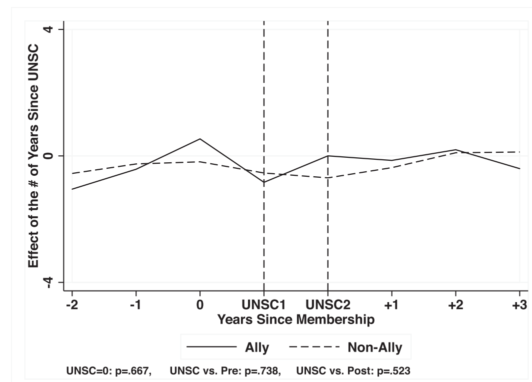
(a) Full Sample (1946-2010)



(b) Truman - Carter (1946-1980)



(c) Clinton (1993-2000)



(d) Bush Jr. (2001-2008)

FIGURE 2. The predicted effect of Council membership for 90th and 10th percentile allies on news coverage during different periods.

TABLE 5. The effect of UNSC membership and alliance on news coverage during the Reagan and Bush Sr. administrations—robustness to additional controls.

	Dependent variable: no. of news articles on human rights abuse				
	(1)	(2)	(3)	(4)	(5)
Dependent variable mean	**4.269**	**4.269**	**4.435**	**5.184**	**4.369**
UNSC × Alliance	−9.23	−8.18	−8.90	−8.37	−9.92
	(2.528)	(2.630)	(2.685)	(3.207)	(3.054)
Controls					
Region−Year FE	N	Y	N	N	N
Polity2$_{t-2}$	N	N	Y	N	N
Amnesty PTS$_{t-2}$	N	N	N	Y	N
GDP$_{t-2}$	N	N	N	N	Y
Observations	1,359	1,359	1,153	1,060	1,300
Clusters	114	114	107	106	110

Notes: All regressions are estimated using poisson regressions and include the baseline controls: UNSC membership × Year FE, Aliance × Year FE, Year FE, and Country FE. The standard errors are clustered at the country level. The number of clusters are reported at the bottom of the table. The sample includes the years 1981–1992. The number of observations vary across columns due to the availability of the control variables.

6.5. *Additional Controls*

In this section, we address the concern of simultaneity bias by controlling for the factors that are most likely to influence news coverage and be correlated with Council membership.

Table 5 column (1) shows our baseline specification results for comparison purposes. Column (2) controls for region-year fixed effects to address the possibility that reader interests and U.S. policy objectives may shift geographically over time. We use the Hadenius and Teorell (2005) definition, which divides the world into ten regions according to geopolitical characteristics. Column (3) controls for the institutional quality of foreign countries that may influence U.S. readers' interests in these countries, the strategic value of their alliance to the United States and a country's ability to obtain Council membership. We use the Polity2 index for constraints on the executive.[32] In column (4), we control for human rights abuse levels as reported by Amnesty PTS. In column (5), we control for income (as reported by the Penn World tables). To avoid endogeneity of the contemporaneous measures, we use a two-year lagged measure of the variables in columns (3), (4) and (5).

The interaction effect is always negative, large in magnitude, and statistically significant at the 1% level. The robustness of our results to additional controls is consistent with our interpretation and goes against the concern that the main findings are unlikely to be driven by omitted variables.

32. Polity2 is an index that measures the autocracy of the executive. It ranges from −10 to 10, where higher values reflect more democratic governments and is reported by the Polity IV Project.

6.6. Mechanisms

In this section, we provide additional evidence consistent with the presence of government distortion, as well as evidence that contradicts the main alternative explanations.

6.6.1. U.S. State Department Reports. Since our preferred explanation for the negative interaction effect is that the degree of government suppression of news coverage of human rights abuses increases with alliance, we examine whether reports of human rights abuses made by the U.S. government decrease with alliance when a foreign country enters the Council. Although it is beyond the scope of our analysis to quantitatively determine the contribution of the different policy instruments used by the U.S. government to distort the news (recall the discussion in Section 2), finding that official government reports respond to U.S. alliance and Council membership in a similar way would provide consistent evidence that our empirical strategy does indeed capture U.S. government objectives.

We examine the U.S. State Department's *Country Reports* on the level of human rights practices of each country. This is an annual publication submitted to Congress and open to the public, including journalists.[33] That journalists are aware that this publication is consistent with the spike in the number articles about human rights printed in newspapers the day after the reports are announced.[34] As the content is entirely determined by the government, it is one of many instruments by which the government can influence the media. Our hypothesis implies that *Country Reports* should favor UNSC members that are allies relative to those that are less allied to the United States.[35] Based on the discussion in Section 2, we would expect that Council membership should cause the State Department to exaggerate human rights abuses of nonallies and favorably understate the abuses of allies.

33. The United States is the only country that systematically releases its reports to the public. The way in which it gathers information is not transparent. However, it is generally assumed that the reports are based on information from government intelligence and diplomatic apparatuses. The wording of the reports also suggest that the information is mostly based on these sources.

34. Online Appendix Figure A.3 plots the average number of articles over time for each day before and after the Country Reports are released for the Reagan and Bush Sr. administrations, which drive our results on news coverage (1981–1992). The release dates vary year to year. There is a buildup in the number of articles starting around five days prior to the release date of State Department Reports and three days prior to Amnesty reports (the reports are often leaked prior to the official release date) and a spike the day following the release that tapers off two days later. This suggests that the reports trigger an increase in news coverage of human rights abuses. However, the total number written between five days prior until three days after the release of both the State Department and Amnesty reports is approximately 50% reports per year. This is less than 12% of the 421 news articles published per year on human rights abuses during this period. Thus, it seems likely that Country Reports served as a reference for journalists writing about human rights, but their release did not determine the timing of most news articles about human rights.

35. The notion that the government uses official publications to promote its views is consistent with the recent study by Latham (2012), which provides evidence that CIA intelligence reports during the Cold War were distorted toward the views of the executive administration.

A potential concern from examining State Department reports of human rights practices is that the reports will capture both government distortion and actual human rights practices. Thus, observing that these reports vary with alliance and Council membership could mean that either the U.S. government distorts reports for strong allies on the Council or that strong allies on the Council improve human rights practices more than less allied countries. To address this, we benchmark the State Department country reports to those by Amnesty International. Since Amnesty is a nongovernment organization, it should not systematically bias its reports based on U.S. government objectives. Both reports are quantified by the *Political Terror Scale* (PTS), where lower scores reflect better practices. The PTS uses a five point coding scheme, where a PTS value of five indicates the most severe abuse. The main dependent variable is thus U.S. PTS minus Amnesty PTS. A positive difference means that the U.S. reports a country as having worse human rights practices than Amnesty reports.

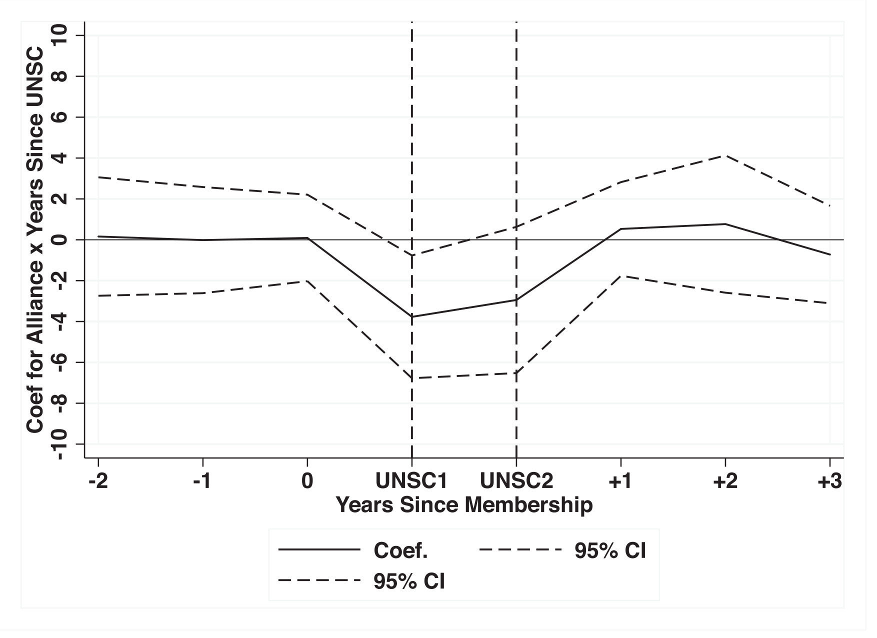
Figure 3(a) plots the interaction coefficients and their 95% confidence intervals during the Reagan and Bush administrations. The exercise is analogous to our year-by-year estimates for news coverage in Section 6.4.[36] It shows that there is no pre-trend leading up to Council membership, the interaction coefficients become negative for the two years of Council membership, and then returns to pre-Council levels.

Figure 3(b) plots the predicted effect of Council membership for 90th and 10th percentile allies. It shows that there is little difference between allies and nonallies prior to or after Council membership. However, for the years on the Council the U.S. increases reports of abuses relative to Amnesty for nonallies. This is consistent with our hypothesis as well as the anecdotal and case study evidence discussed earlier in Section 2 about how the government behaved during this period.

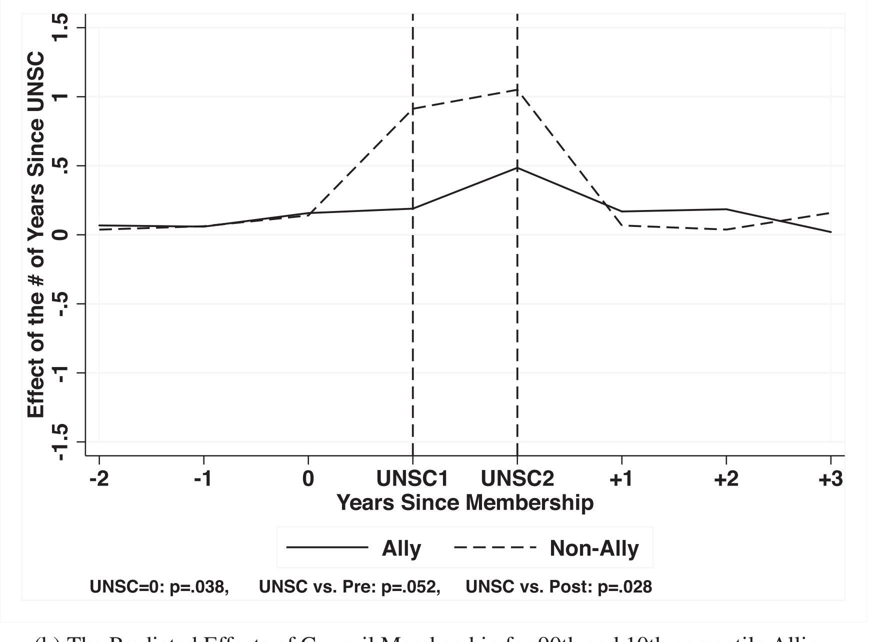
In Online Appendix B and Online Appendix Figures A.6(a)–A.6(d), we present the results for other time periods for completeness. The coefficients and standard errors for the figures are shown in Online Appendix Table A.9.

6.6.2. Alternative Interpretation. The main alternative explanation for our results is the possibility that strongly allied countries improve actual human rights practices (relative to weaker allies) when they enter the Council. This seems unlikely given that Online Appendix Table A.10 shows that there is no effect on human rights practices as reported by Amnesty International. However, to investigate this possibility further, we also examine institutional outcomes that are potential correlates of human rights practices. We use all of the measures that are available to researchers today that are reported by nongovernment agencies. These include the Civil Liberties and Political Rights indices reported by Freedom House, the Polity2 Index for constraints on the

36. Note that we add a control for Amnesty PTS and use the same sample as in the baseline specification for PTS. This does not introduce endogeneity since we show that the interaction of Council membership and alliance has no effect on Amnesty PTS, but it improves the precision of our estimates. See Online Appendix B for a discussion. Also note that for the Reagan–Bush period PTS estimates, we drop Peru 1982, which is an outlier. Our main result on the interaction of UNSC and alliance is similar without this omission.



(a) The Coefficients for UNSC × Years Since Council Membership



(b) The Predicted Effects of Council Membership for 90th and 10th percentile Allies

FIGURE 3. The effect of UNSC × years since Council membership on Human Rights Country Reports (U.S. PTS—Amnesty PTS) during the Reagan and Bush Sr. administrations.

TABLE 6. The effect of UNSC membership and alliance on human rights measures and institutional quality during the Reagan and Bush Sr. administrations.

	Dependent variable: institutional quality and conflict indicators				
	(1) Freedom house civil liberties	(2) Freedom house political rights	(3) Combined polity score	(4) UCDP conflict dummy	(5) Principal component
Dependent Variable Mean	**4.71**	**4.71**	**0.35**	**0.30**	**0.001**
UNSC ×Alliance	1.71	−2.35	−0.18	−0.44	−0.40
	(2.957)	(1.536)	(0.816)	(0.426)	(2.140)
Standardized Coefficient	*0.02*	*−0.04*	*−0.01*	*−0.03*	*−0.01*
Observations	1,132	1,132	1,243	1,212	1,103
Mean Dependent Variable	4.766	4.756	0.338	0.301	0.00121
R-squared	0.855	0.858	0.778	0.761	0.860
Clusters	114	114	114	111	111

Notes: All regressions are estimated using OLS and include the baseline controls: UNSC membership Year FE, Alliance × Year FE, Year FE, and Country FE. The standard errors are clustered at the country level. The number of clusters is reported at the bottom of the table. The sample includes the years 1981–1992. The number of observations varies across columns due to the availability of the dependent variables.

executive, and the incidence of civil conflict that results in 25 or more battle deaths as reported by UCDP/PRIO. For Polity2, we follow the literature and use a dummy that equals one if the index is less than zero to indicate that a country is an autocracy.[37] Since these variables are indices and can take negative values, we use OLS to estimate the baseline equation, equation (1).

Table 6 shows that the interaction effect is statistically insignificant for all outcomes. To help compare the magnitudes of the effects across the different outcomes, we present the standard deviation change in the dependent variable that results from a one standard deviation change in the interaction term. The standardized effects are similarly small in magnitude across outcome variables.In terms of absolute value, the standardized effect ranges from −0.04 to 0.02. In the last column, we examine the first principal component of all of the institutional measures. As before, we find no effect.

These estimates are inconsistent with the alternative interpretation that our main result is driven by improvements in the relative human rights practices of strongly allied countries when they enter the Council.

6.7. Total News Coverage

A potential concern for our interpretation is that the main results are driven by an increase in total news coverage rather than a disproportional increase in news about

37. These variables are provided by the Quality of Government (QoG) dataset. In addition to what is presented here, we examine a large array of other variables reported by the QoG dataset and find no effect on any of them. These results are available upon request.

human rights abuses. This would then suggest an alternative mechanism: Council members that are less allied with the United States may simply be more interesting to readers. If newspapers are space constrained (or if readers are attention constrained), then an increase in coverage of nonallies would also necessitate a reduction in coverage of allies. Our prior is that this is unlikely since nonallies would presumably be less interesting to American readers than allies. Nevertheless, we can investigate this by examining the share of all coverage that is about human rights abuses as the dependent variable. We examine the number of articles about human rights abuses as a fraction of the total number of articles about a country in a given year as the dependent variable. The number of all articles is constructed by searching for articles that contain a country's name in a given year. The data show that during 1981–1992, a small percentage of all articles about a country are about human rights abuses on average (0.26%). Since the dependent variable is a fraction, we estimate this regression using OLS. The interaction coefficient is −0.02 and significant at the 5% level (not presented in tables).

Similarly, we can examine the effect for each year since Council membership by estimating equation (2) with the fraction of human rights abuse articles as the dependent variable. Again, we use OLS for this estimate. The predicted effects for the 90th and 10th percentile allies are plotted in Figure 1(c). We observe the same pattern as for our main dependent variable.[38]

The results show that our main findings are not solely driven by changes to total coverage. This supports our interpretation.

6.8. Newswires

In considering the mechanisms driving our result, it is also interesting to note that we find a very large effect on the coverage of human rights abuses by newswires, which on average report more than four times the number of stories on human rights abuses than any of the newspapers in our sample. The interaction coefficient is −13.72 and is statistically significant at the 1% level.[39] Since newspapers often pick up stories from newswires, this suggests that one effective way for the government to distort the news is to distort coverage by newswires. Unfortunately, we are unable to investigate this more rigorously because it is not possible to systematically distinguish news that are picked up from newswires from other articles.[40]

38. The coefficients and their 95% confidence intervals are plotted in Online Appendix Figure A.4(e).

39. The estimate and descriptive statistics for newswires are shown in Online Appendix Table A.11 column (1).

40. This means that newswires are not part of the total number of articles that we examine as our main dependent variable. We also estimate the interaction of Council membership and dummy variables for each year since Council membership separately for each of the five main newspapers and newswires. Online Appendix Figures A.8(a)–A.8(e) plot the predicted effects of Council membership for each year since Council membership for the 90th and 10th percentile allies for our five main newspapers; Appendix Figure A.8(f) plots the predicted effects for newswires. We see a similar pattern as with our main results.

6.9. Additional Results

In addition to the results presented here, we separately examine the extent of the distortion for each newspaper in our sample. We find that the extent of the distortion is positively correlated with the quality of the newspaper, which we proxy with the number of Pulitzer Prizes for foreign coverage. That the government would target the highest quality of newspaper or the paper with the largest circulation is consistent with our interpretation and the model presented in the Appendix. However, these results are merely suggestive since they effectively rely on only five observations (newspapers). See the Online Appendix C.

7. Interpretation and Alternative Explanations

The empirical findings that Council membership reduces news coverage of human rights abuses for strongly allied countries, whereas increasing coverage for countries that are not allied during the Reagan and Bush Sr. administrations is consistent with the presence of demand effects and government distortion in the framework discussed in Section 3. Within the context of this model, the negative interaction effect of Council membership and alliance is consistent with the presence of government distortion (for the Reagan and Bush Cold War administrations). That said, it is beyond the scope of our study to be conclusive about the mechanisms driving the empirical results. In this section, we discuss several alternative explanations and the necessary conditions for reconciling them to the empirical results.

One natural alternative explanation is that the U.S. government used news coverage of human rights abuses to buy votes on the Security Council. This would cause Council membership to reduce news coverage of strongly allied countries if the most strongly allied countries in the General Assembly (from where we take our measure of alliance) are the marginal voters on the Security Council. Although such mechanisms are possible, the simplest versions of it seem inconsistent with our finding. In particular, standard vote buying models predict that the U.S. government should mainly target the "swing voters"—countries that are most likely to change their voting behavior in response to favorable coverage, not countries already inclined to do so. Such models are difficult to reconcile with the finding that Council membership increases news coverage of human rights abuses for countries that are not allied with the United States (see Figure 1(b)).[41]

Relatedly, it is possible that newspaper readers themselves feel nationalistic about Council members and endogenously demand more positive news coverage about allies

The coefficients and standard errors for newswires are reported in Online Appendix Table A.12 column (6).

41. We also note that Kuziemko and Werker (2006) find that aid increases for Council members during years that are strategically important to the United States and interpret this as vote buying. Motivated by this, we examine U.S. foreign aid as the dependent variable in our baseline specification and find that the interaction coefficient is small in magnitude and statistically insignificant. Thus, there is no obvious evidence of vote buying.

and more negative one about nonallies. In this case, a profit maximizing newspaper may endogenously respond by slanting its coverage as, for example, in the works of Gentzkow and Shapiro (2006) and Mullainathan and Shleifer (2005). We are not aware of any evidence that readers' preferences adjust this way, but the literature on readers' preferences is very limited, and more work is needed in this area.

If one departs from the standard assumption in the literature that U.S. news outlets are profit maximizing, then our results are also consistent with journalists or the management of newspapers distorting coverage. For example, they may feel patriotic about allies that are on the Council—that is, it is in the best interest of the United States to not portray important allies poorly. This is an interesting avenue to pursue in future research. Another possibility is that newspapers rely on U.S. State Department information and their pattern of coverage is primarily driven by State Department news releases. It seems reasonable that profit-maximizing newspapers would put some weight on these reports since they contain valuable information that is otherwise costly to acquire, which could lead to distortions even in the absence of any direct government influence or interactions. This mechanism by itself, however, does not explain why the U.S. department chooses to increase unfavorable news coverage for nonallies and decrease coverage for allies once they get on to the Security Council. Our model provides a possible rationale for why the State Department would choose such a policy.

8. Conclusion

This paper explores the possibility that the government can systematically distort news coverage from independently owned media outlets in the United States. Using data from 1946 to 2010, we show that membership on the UN Security Council increased news coverage of the human rights abuses of foreign countries when they are not politically allied to the United States. In contrast, for countries that are strongly allied to the United States, membership reduced news coverage of bad behavior. We argue that these results are consistent with government distortion. However, these distortions are only present during the latter part of the Cold War under the Reagan and Bush Senior administrations. Interestingly, this is also the period for which there is a large body of archival evidence documenting the government's intent and methods for distorting news coverage of human rights behavior according to foreign countries' strategic alliance with the United States.

These results provide novel and rigorous evidence that government distortion can systematically exist (albeit for a finite period of time) in a highly competitive media market among independently owned media. For policymakers and practitioners, our results may produce mixed feelings of unease and reassurance. On one hand, the presence of systematic government distortion in U.S. media content is consistent with the fear that government distortion can impede the media's ability to monitor the government on behalf of its consumers. That this can occur in a democratic regime known for media independence suggests that market forces are not always a sufficient guarantee against government influence. Note that the United States has one of the largest and most competitive media markets in the world (Djankov et al. 2003). On the other hand, that we only find distortions for a twelve-year period suggests that perhaps government distortion would not have been sustainable over time.

The results and limitations of our study suggest several avenues for future research. First, as data from more recent years become available, it would be interesting to examine whether similar effects exist for the Obama administration during its wars in the Middle East, when human rights of belligerent nations have received significant press attention. Another important line of inquiry is to understand the conditions under which a democratically elected government can systematically distort the news. Our finding that there is distortion during the latter part of the Cold War together with the recent work of Gentzkow et al. (2015), which finds that historically in the United States, the government in power has little effect on news composition, shows that there is variation in government distortion even within one political system.

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Supplementary Data

Supplementary data are available at *JEEA* online.